On the Substance Exhibited at the British Association, Brighton, by Mr. P. L. Sclater, and stated to be the Ossified Notochord of a Fish

NEARLY every one frequenting the Zoological Section at the British Association at Brighton must have seen and been puzzled by a substance exactly resembling in external appearance a slender willow twig when perfect, two feet or more in length, and pointed at both ends.

This substance was exhibited by Mr. Sclater, and pieces of it were freely distributed by him for examination.

I was, unfortunately, not present when he read his paper on the subject; but I gathered that he had said that the substance had been described to him by the person who sent it as occurring in the back of a fish, and that Mr. Sclater called it an ossified notochord. A drawing of the fish was exhibited.

I further heard that Mr. Gray, of the British Museum, regarded the substance as the axis of one of the Pennatulidæ, and that this opinion was held by several other naturalists also.

I first became acquainted with the substance at the Kew Herbarium, where a piece of it was shown me by Mr. Berkeley, it having been given to him by Prof. Thiselton Dyer; and I was told that Dr. Hooker had examined it with the microscope, and rejected it as certainly not vegetable.

It was almost impossible to conceive of the substance being the notochord of a fish. No fish's notochord is composed of longitudinal fibres, nor has a structure at all resembling that of the substance in question; and moreover a notochord in such a fish as a lamprey, in which it is persistent, is much thicker in proportion to its length than are these calcified rods. Further, the tendency is for a notochord to ossify peripherally, and form rings of bone, not a hardened central core.

On reaching Oxford from Brighton, I got Mr. Robertson to give me a specimen of *Funicularia quadrangularis*, one of the Pennatulidæ, which was preserved in spirits. I found it had a long slender flexible core, exactly similar in appearance to Mr. Sclater's substance, but quadrangular in section instead of circular. The core was about two feet and a half long, and pointed at both ends. Microscopical examination of longitudinal sections of the core, when treated with acetic acid, gave off carbonic acid in quantities, and showed a structure almost exactly resembling that observed under similar circumstances in Mr. Sclater's substance.

I then looked into the literature of the subject, which fully confirmed me in the opinion that the substance in question is the core of one of the Pennatulidæ. A few statements, culled from the two works I consulted, may be interesting to the readers of NATURE. The works were "Anatomisch-systematische Beschreibung der Alcyonarien," von A. Kölliker, Erste Abtheilung: Die Pennatuliden. Erste Hälfte (Frankfort: C. Winter, 1870). "Icones Histologicæ, oder Atlas der Vergleichenden Gewebelehre," herausgegeben von A. Kölliker. Zweite Abtheilung, Erster Heft. Die Binde-substanz der Cœlenteraten, p. 158 (Leipzig: W. Engelman, 1866).

The Alcyonariæ, a sub-order of polyps, are divided into three groups:—(1) Alcyonidæ; (2) Gorgonidæ; (3) Pennatulidæ.

The Pennatulidæ consist of hard and soft parts. The hard parts appear in most varieties in the form of an inner calcified axis, which in size and position is like that of the Gorgonidæ. It is to be considered as calcified connective tissue, is entirely and completely enclosed within the substance of the polyp colony, and is pointed at both ends.

The Pennatulidæ are thus divided:—

- I. Pennatulidæ with polypbearers bilaterally symmetrical.
  - A. Polypbearers feather-shaped in Pennatuleæ.
  - B. Polypbearers leaf-shaped in Renillaceæ.
- II. Pennatulidæ, with polyps arranged radially.

The Pennatuleæ break up into (1) Penniformes; Pennatuleæ with a well-marked feather-shape; (2) Virgularieæ; Pennatuleæ with a long, narrow polypbearer, and small leaves or polyps resting immediately on the axis. To this latter group belongs the genus *Funicularia*, and probably also the genus to which Mr. Sclater's specimen belongs.

With regard to the fine structure of the hard axis of Pennatulidæ, I have gathered the following from Kölliker's "Icones," p. 158.

The axes of Pennatulidæ consist of calcified horny substance, arranged in concentric lamellæ about a central core. The lamellæ are pierced by peculiar soft radial fibres, which, however, are well defined in certain species only. The organic basal substance shows an extremely well marked fibrillar structure. The axes are less firmly calcified than those of the Gorgonidæ, and are thus for the most part able to be cut with a knife and bent.

According to the analyses of Fremy (Ann. de Chimie, 1855, t. xliii. p. 98), the axis of *Pterocides spinosum* contains from 31 to 40 per cent. mineral matter, and that of *Pennatula rubra* from 45 to 48 per cent.

A drawing is given by Kölliker of a transverse section of the axis of a Virgularia (*Lygus mirabilis*) prepared by grinding, which shows a white central core, surrounded by a broad brownish cortex, which is marked with concentric and radial lines.

If the axis of a *Lygus* be treated with acetic acid, a development of carbonic acid takes place. It becomes soft, and allows the following structure to be made out:—

The bulk of the axis consists of a fibrous tissue which resembles ordinary fibrillar connective tissue in the most deceptive manner, and consists of very fine fibrillæ, which run parallel to one another in a wavy fashion, and which can be isolated from one another. On the surface of the axis is a yellowish cuticle.

Drawings are given of longitudinal sections of the axis of *Lygus mirabilis*. It shows the peculiar broad transparent radial fibres crossing the finer longitudinal ones. In another figure of a similar preparation from *Funicularia quadrangularis*. these radial fibres are less marked, but the cavities containing them appear as oval apertures in the section.

Reference is made to Quekett, Lectures on Histology, II., and Histological Catalogue, I., where the structure of the axes of *Pteroeides*, *Lygus*, and *Funicularia* is described, but the radial fibres mistaken for canals.

I think any one who has examined Mr. Sclater's substance, and very many have had such an opportunity owing to his kindness in distributing pieces, will find that both in external characteristics and internal microscopical structure, it conforms very closely to the description given here from Kölliker of the axis of the Pennatulid. I have sent the Editor of NATURE some pieces of the axis of *Funicularia quadrangularis* in case any one cares to compare the two substances, and has not the material at hand. In the mean time I cannot but conclude that Mr. Sclater has been misinformed, and that we are very unlikely ever to see that very marvellous fish in the flesh.

H. N. MOSELEY

Ocean Currents

HAVING just returned from a sojourn of nearly two months amongst the White Mountains, I am now for the first made aware of the publication, both of my last note on Ocean Currents, and also of Mr. Croll's reply. I have not been disposed to enter into an extended discussion of this subject, knowing that it cannot be properly treated without the use of mathematics, in short essays suited to NATURE, and doubting whether the discussion could be made either acceptable to its Editor or edifying to its readers. In my last note, therefore, I endeavoured to be as brief as possible, and considered only the more simple form of the conditions of the problem, as expressed by differential equations, showing the relations between the forces, resistances, and the differentials of the motions, and showed that the deflecting force eastward exerted upon a pound of water or any body in moving toward the pole with a velocity of one mile per day, and which must be sensibly the measure of the resistance of friction, is of the same order near the parallel of 45° as the action of gravity on the same body upon a gradient of 6 ft. from the equator to the pole; and from tidal considerations it was inferred that the resistances to the slow motions of ocean currents may be very much less than the action of gravity upon any body apon a gradient of 6 ft. in the distance of a quadrant.

If, instead of considering the differential equations of any problem, and endeavouring to satisfy them directly, we adopt the less simple method, and consider the integrals of these equations, and endeavour to satisfy them directly, the method, though less simple, is entirely legitimate, and we should obtain the same results. This is substantially the method adopted by Mr. Croll; and from considering the problem in this way, he comes to the conclusion that the deflecting force eastward, which is the measure of the resistance, is at least 1,500 times greater than the action of gravity on a gradient of 6 ft. from the equator to the parallel of 60°; and as the velocity of the pound of water eastward, and that toward the pole, are probably about of the same order, and consequently the resistances, he justly infers that the resistance to the motion toward the pole must be overcome by

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some other force than that of gravity due to the assumed gradient. But it may be shown by the other method of considering the problem, by assuming a motion towards the pole of one mile per day, that the forces which overcome the resistances in both directions are about of the same order. This very great difference in the results obtained from the two methods of considering the problem, indicates that there is some great fallacy somewhere which needs looking after.

Mr. Croll is misled by adopting the erroneous principle that the amount of work performed by gravity upon a falling or descending mass is in all cases expressed by the mass multiplied into the height through which it descends, or that the foot-pound is a unit of work. The amount of work required to give velocity to any body, or overcome any kind of frictional resistance, is expressed by the intensity of the effort, regarded as constant, multiplied into the time of action. The intensity of the effort of any force, as of gravity, is explained by the mass multiplied into the velocity which such force can produce in a unit of time. If we, therefore, put g, m, t, and v for the force, the mass, the time, and the velocity respectively, we shall have, putting W for the amount of work performed by gravity,

$$(1) \quad W = mgt = mv,$$

that is, the amount of work performed is expressed by the momentum. Now this amount of work is stored away in the moving body, and remains until it is used in overcoming resistance of some kind, as friction or the inertia of other bodies, and W is exactly the expression of the working power which has been communicated to it. But if the moving body has been subject to resistances, as of friction, during the time t, then we shall have

$$(2) \quad W = mv + ft$$

putting f for the coefficient of friction, and supposing it to be constant. In this case mv expresses the amount of work left which has not been expended in overcoming the friction during the time t, and of course in this case we cannot get all the work back again which has been expended, at least mechanically.

If we now suppose a body to fall in a vacuum through the space s, if the amount of work performed by gravity upon it, and the working power communicated to it, is expressed by the mass multiplied into the height through which it has fallen, we shall also have

$$(3) \quad W = ms = \tfrac{1}{2}mgt^2 = \tfrac{1}{2}mvt.$$

Hence, comparing the preceding expressions of W, we have $mv = \frac{1}{2}mvt$, which is impossible; and therefore, if the former expression of W is correct, the latter is not.

Again, illustrating by a special case, if we suppose a body, of which the momentum is mv, to move upon a level plane without friction, and the plane to curve up in the direction of motion, and also suppose another body with half the mass and double the velocity, of which the momentum is $\frac{1}{2}m \times 2v = mv$, m and v being the mass and velocity of the first body, it must be admitted that the amount of labour expended in giving both bodies the momentum mv is exactly the same, in the latter case the intensity of the effort being half as great, but the time of action twice as long; but the momentum will carry the latter up the slope to a height four times greater than that of the former, and after descending again to the plane, both will have the same momentum, and the same amount of labour would be required to bring each to rest, and consequently both have the same power of doing work. But the mass of the latter multiplied into the height through which it has descended is double that of the former, and hence these products do not express the power of doing work which gravity has communicated to them.

In the case in which the descending body is resisted by friction, we have seen (2) that neither the mass multiplied into the height of descent, nor the momentum mv which the body has on arriving at the level plane, expresses the whole action of gravity, and the resistance fv may be so small as not to affect sensibly the amount of work expressed by mv, or it may be so great that mv may be neglected, in comparison with ft. The value of ft may also be so great that the value which mv would have (1) in the case of a free body falling through the space s, would be almost infinitely small in comparison with the whole expression of W (2). In the various cases, therefore, which may be supposed, in which friction may be either very small or very great, so that in the former case the effect of the resistance might be scarcely sensible, and in the latter it might take the action of gravity a long time to drag the body down through the space which it has to descend, we cannot suppose that in all these cases the whole action of gravity is expressed by the same number of foot-pounds, supposed to be units of work.

If, therefore, Mr. Croll's pound of water were moved from the equator to the parallel of 60° by the action of gravity without any resistance, the momentum which it would have on arriving there would express the work done by gravity upon it, and not the six foot-pounds, and the work would be done in a very short time in comparison with the time in the real case of nature; but when it is dragged down there through all the resistance which it suffers, at the rate of a mile per day, as we have supposed, the amount of labour which gravity performs is very many times greater than that expressed by the momentum which the pound of water would have on arriving there without resistance; and with regard to the six foot-pounds, we have seen that the work is no more comparable to them than a surface is to a solid.

Again, if we suppose the gradient upon which gravity acts to be only one foot in the distance instead of six, and the resistance to the pound of water to be as the velocity, then the water would move with only one-sixth of the velocity in the other case, and the water would be six times as long in reaching the parallel of 60°, but the energy of the action of gravity would be only one-sixth as much, and hence the work would be the same, being carried on six times as long in the latter case with one-sixth of the energy. But then the same work would be represented by one foot-pound, if that is a true unit of the work instead of six. In this case also the deflecting force eastward would be only one-sixth as much, but the time being six times as long, the same amount of work would be done, and this would be sensibly the same as that which would be required to give the pound of water a velocity of about 760 miles, as Mr. Croll has it, but really double that amount.

I am well aware that in the action of machines in which force is balanced against force, and consequently the times of action are the same, the amount of work may be expressed by the forces multiplied into the spaces through which they act; but in all cases in which the times differ, the amount of work cannot be expressed by any unit into which the element of time does not enter. The amount of work required to produce a velocity of 760 miles per hour is a function of the time, and proportional to the time where the force is constant, and cannot be measured by foot-pounds.

With regard to the argument based upon M. Dubuat's experiment, the matter briefly and fairly stated stands thus: according to the experiment, water will not flow unless acted upon by a force equivalent to that of gravity upon a given gradient, which makes the force required to move it about fifteen times greater than the horizontal component of the moon's disturbing force which produces the tides. But this force of the moon does move the water of the ocean, and therefore M. Dubuat's experiment is not applicable to water of great depth as of the ocean, and the argument fails. It is true, as Mr. Croll states, that the two cases of motion are somewhat different; in the case of the tides the water from top to bottom flows in the same direction, while in the other the upper and lower strata flow in contrary directions, and the resistances to the lower motions are no doubt greater. Six of the nine feet therefore of Mr. Croll's gradient should probably be given to the lower currents, and only three to the upper ones. But Mr. Croll admits that in the case of the tides a gradient of one inch is sufficient to move the water. A gradient of three feet only, therefore, ought to be sufficient to move the upper half, which would correspond somewhat to the case of tides in an ocean of half the depth. The observations of Col. Graham show that the water of Lake Michigan, about 700 feet deep, readily yields to the moon's disturbing force which causes a tide at Chicago with a range of nearly two inches.

Cambridge, Mass., Sept. 7 WM. FERREL

Spectral Nomenclature

IT seems almost absurd that a subject of such interest, and, as I think, importance, as that of Spectral Nomenclature should be discussed from opposite sides of the globe alone; so it may be hoped that it will not have been allowed to end with Prof. Young's remarks upon the one or two points in which he differs from me, but will have been taken up by others at a less distance. There is a great deal more to be said about it; but probably I should not have troubled you again just yet but for the obligation I feel to disclaim credit which he gives me for what is not mine.

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